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Interactive comment on "Role of CO_2, climate and land use in regulating the seasonal amplitude increase of carbon fluxes in terrestrial ecosystems: a multimodel analysis" *by* Fang Zhao et al.

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My responses to the referee are listed below. Responses are in bold.

Review of the article "Role of CO2, climate and land use in regulating the seasonal amplitude increase of carbon fluxes in terrestrial ecosystems: a multimodel analysis" by Zhao et al. This article presents interesting results in the scope of BG. It can be published after the authors haven taken care of the minor issues stated below. Therefore, I've asked for minor revisions only. However, I'm convinced that the paper could be improved substantially by a small effort, if the discussion would be extended in the

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Discussion paper

two directions I try to describe in the following: The study has two parts. In the first part it is evaluated, if nine global vegetation models (from the TRENDY project) can reproduce the seasonal cycle of land carbon fluxes as derived from atmospheric inversions (figure 1 to 3). In the second part of the article these models are used to investigate trends in the seasonal cycle amplitude of land carbon fluxes. This is done by separating the contributions to the trends for three major forcing factors: rising atmospheric CO2 concentration, trends in climate, and land use/cover change (figure 5 to 9).

Thank you for the positive feedback and nice summary.

In view of the large uncertainties in land carbon cycle model results, I appreciate this study very much. Combining model evaluation and factorial analysis will probably provide a better understanding of the seasonal cycle carbon fluxes (as also mentioned by the authors in the introduction, page 3 line 20-21). Unfortunately, this potential is not utilized. The findings about the performance of each model are (almost) not mentioned in the discussion of the second part. For example, the results shown in figure 5 could be discussed in view of the evaluation presented in figure 1. Some questions like the following could be posed and answered: are the models that successfully simulate the seasonal cycle of carbon uptake more similar in the CO2 fertilization factor than the models that fail to reproduce the seasonal cycle? Or is this true for the climate factor? Analogously, figure 6 could be compared with figure 2.

Thanks for pointing this out. This aspect was indeed overlooked in the original version of this paper. In the revised version, we have incorporated some interesting similarity among the four models with similar mean seasonal cycle of global carbon flux inversions. Specifically, we added the following:

1. Global trend: added this paragraph on P9 before section 3.3.1:"The four models (CLM4.5BGC, VEGAS, LPX-Bern and ORCHIDEE) that simulate a more realistic mean global FTA seasonal cycle (Figure 1) are also relatively close in global FTA seasonal amplitude, clustering around an increase of 14±3% during

1961-2012. Furthermore, they all suggest land use/cover change contribute positively to global FTA seasonal amplitude increase. On the other hand, four of the remaining five models (OCN, LPJ, JULES, VISIT) show a much larger rate of increase (26±3%), but given the fact that these four models underestimate the mean amplitude by about 50%, the absolute increase in global FTA seasonal amplitude is actually similar (about 5 PgC y−1) between the two groups of models. ISAM is an exception, it both underestimates the mean global FTA seasonal amplitude and has the lowest rate of amplitude increase."

2. Consistency in the climate factor for northern temperate region, change the original sentences starting on P9 in L5 to:"In the Northern temperate (23.5-50N) region, climate change alone would decrease the FTA amplitude—this is consistent among the four models with realistic mean global and Northern temperate (Figure 2) FTA seasonal cycle simulation, but is not the case for JULES and LPJ (Figure 6). Such decrease is possibly related to mid-latitude drought (Buermann et al., 2007), which is consistent with findings by Schneising et al. (2014), who observed a negative relationship between temperature and seasonal amplitude of xCO2 from both satellite measurements and CarbonTracker during 2003-2011 for the Northern temperate zone."

3. At the end of section 3.3.3:"While most models indicate land use/cover change in Southern tropics (Amazon is probably the most notable region) decrease global FTA amplitude during 1961-2012, LPJ suggests it would cause a large increase in the amplitude instead, possibly related to its different behavior in simulating mean seasonal cycle of carbon flux for that region (Figure 2d)."

Finally, we want to understand by such studies, why the magnitude of the seasonal cycle in atmospheric CO2 increased. Additionally, we want to confirm that vegetation models respond reasonably to global warming and increasing atmospheric CO2. The

self-evident way to reach both aims is to evaluate models and then to analyse their results by taking into account this evaluation. The later step is incomplete in the current version of the text as the evaluation is not considered in the discussion of the forced simulation results.

We have now incorporated evaluation of mean seasonal cycle in discussing the factorial attribution results as explained above, and this hopefully make the revised version more complete. We cannot confirm if all vegetation models respond reasonably to global warming and increasing CO2, as there are important differences in models' sensitivity to them (therefore not all models can respond realistically to changes of CO2 and climate in all regions), especially at regional level, as also indicated from the results in this study. In the concluding remarks of this manuscript, we have outlined the future study that we would be very much interested to pursue, and encourage the community to work on. Such work will build on the important regional differences identified in this study and would allow the models to be evaluated extensively and comprehensively, which would help to reach both of the suggested aims.

Another important comment I would like to add concerns figure 10. This shows a moderate correlation between the change in net land carbon uptake and the increase in the amplitude of the seasonal carbon fluxes as simulated by the different vegetation models. The authors mention that this cross-model correlation may be used to constrain land carbon uptake (page 11 line 9). I think, this is the key motivation to investigate the seasonal amplitude of carbon fluxes and it should be also mentioned in the introduction. Furthermore, the authors claim for more research on observed CO2 fluxes and atmospheric transport on a regional scale to substantiate this finding (page 11, line 11-13). I agree, but the obvious next step is to further investigate the results of the model ensemble, how the correlation is simulated. A factorial analysis of the long-term carbon uptake should be performed and then compared with figure 5. Thereby, it could be specified, which factor contributes to what extent to the correlation. Probably this

is beyond the scope of the article, but at least this opportunity should be mentioned. And, this kind of analysis is commonly denoted by the keyword "emergent constraint". It would be good to cite a reference, perhaps the paper of Cox et al 2013.

The idea of using the keyword "emergent constraint" was indeed discussed among co-authors at an earlier stage of the paper, however we decided against it as we have not completely understood the mechanisms behind this correlation, and we already have lots of material in this paper at that point. The obvious next step as you mentioned is indeed to also perform a factorial analysis of the long-term carbon uptake, however since figure 10 does not actually represent the key of this paper, but rather a very interesting observation (similar observations were also made in Ito et al. 2016; Zhao and Zeng 2014) that has potential in future studies. We are very much interested to further explore this opportunity later, but for now, we decided to simply mention this as suggested after "in aggregated global values": "A factorial analysis of the long-term carbon uptake could help to determine which factor contributes to what extent to this correlation."

Minor scientific issues - Why is the exceptional result of VEGAS concerning the CO2 factor mentioned in the abstract (page 1 line 32)? It is not a key finding.

We do believe the result of VEGAS is important to be mentioned here since it is the only model that both simulates a realistic seasonal cycle of carbon flux and indicating CO2 is not the most important factor in the amplitude increase. This is possibly also associated with the weaker CO2 fertilization effect in VEGAS, whereas CO2 fertilization effect is strong in many other models. There are certainly many arguments on the strength of CO2 fertilization in real world, which is one of the most important issues in carbon cycle science but beyond the scope of this paper. Without including this key disagreement, the readers could be left only with the impression that a majority DGVMs agree on CO2 being the most important factor, which would be the opposite of what this paper tries to convey:

the models disagree on the importance of the three factors, despite that they generally agree on the overall trend, and we should validate them at regional scale in future studies.

- "is a good indicator of terrestrial ecosystem dynamics" (page 2 line 11). This is a too general statement. It does not help in the line of argument. I would skip it.

Deleted as suggested.

- It would be nice to mention in section 2.1 that the TRENDY model simulations are offline simulations driven by climate data (and other input like atm. CO2 concentration) and that the models are not coupled to general circulation models. Of course this can be deduced from appendix A, but it should be also clearly stated in the main text as it is important for its perceivability (e.g. the differences in the results between the models can not be due to weather noise).

Good suggestion. The sentence in section 2.1 now reads:
"A set of three offline experiments driven by either constant or varying climate data and other input such as atmospheric CO2 and land use/cover forcing were designed in the TRENDY project to differentiate the role of CO2, climate and land use (Table 2)."

- The authors assume that the models simulate the effect of CO2, climate, and land use "linearly" (page 5 line 16-22). I think, "linear" is missleading in this context. It is more about synergy terms. For example, the trend in S2 minus S1 includes the climate effect and the synergy of CO2+climate. Furthermore, I'm not convinced that the synergy terms are all unimportant. Therefore, I propose to simply state that the climate effect and the synergy of CO2+climate together are called "climate" for simplicity in the rest of the manuscript. Without a discussion whether the synergy terms are negligible or not. And, of course, analogously for S3 minus S2.

Good point. Even though we have tested that the synergy terms are very small

in one model, we do not have the means to test that for all models. Following your suggestion, we have changed the relevant text as below:
"The effect of CO2 on the relative amplitude change is represented by trend of S1 (CO2 only) results, the S2 (CO2+Climate) results show a trend that is the sum of CO2 and climate effects, and the S3 (CO2+Climate+Land Use/Cover) simulations include trends from time-varying CO2, climate and land use/cover change (abbreviated as LandUse for text and figures). For simplicity, the effect of "climate" as used in this paper includes the synergy of CO2 and climate, and similarly the effect of "land use/cover" also includes the synergy terms. Therefore, effect of CO2, climate and land use/cover are then quantified as the trend for S1, trend of S2 minus S1 trend, and trend of S3 minus S2 trend, respectively. Note that the synergy terms are likely small in some of the current generation dynamic vegetation models, such as shown in previous sensitivity experiment results (Zeng et al., 2014)."

- Please replace "Q10 value" (page 6 line 22) by "temperature dependence of heterotrophic respiration". Not everyone is familiar with this shortcut.

Replaced as suggested.

- Concerning the temporal trend in the seasonal amplitude in the late 90s (page 8 line 9): I can not deduce from figure 7 that half of the models exhibit a decrease, but it is obvious that the model ensemble shows an increase.

We agree that the model ensemble obviously shows an increase, however that is largely contributed by the JULES model. For the rest of the models it is more likely a half-half split. Also "trend" is probably not the best word here to describe a change in a few years. Therefore, we have now stated the model names (LPJ, OCN, ORCHIDEE, VEGAS) where at least some sort of decrease in the late 90s is found, even though the amplitude of this change maybe small and there is a rebound in some that is larger than the observation records indicate. We have

also changed the word “trend” to “change”. However we would like to avoid too detailed discussion here.

- The models agree in the general seasonal amplitude increase, but they disagree in the contribution of the climate factor as well as the land use factor to the seasonal amplitude trend (see figure 5). I think, this disagreement even in sign should be mentioned in section 3.3. In the following subsections this important fact may be overseen due to all the details stated there.

Thanks for pointing this out. This message should indeed be put in a more prominent location. Therefore, the first sentence in section 3.3 is now changed to:
“Models agree on increase of global FTA seasonal amplitude during 1961-2012, but they disagree even in sign in the contribution of the different factors (Figure 5).”

Typos etc. page 1 line 25: replace "during 1961-2012 for its seasonal cycle and amplitude trend" by "for its seasonal cycle and amplitude trend during 1961-2012" page Âň1 line 31: replace "is a stronger" by "is the strongest" page 2 line 9: replace "of CO2 seasonal cycle" by "of atmospheric CO2 seasonal cycle" page 2 line 25: replace "in understanding the contribution of various mechanisms" by "to disentangle effects of various mechanism" page 3 line 1: replace "instead of dynamic vegetation models" by "instead of biases in dynamic vegetation models" page 4 line 14: replace "A direct comparison with fluxes from process-based models are monthly" by "Fluxes from process-based models can be directly compared with monthly" page 4 line 29: replace "The seasonal amplitude of Mauna Loa Observatory or global CO2 growth rate and fluxes from model simulations and inversions are computed" by "The seasonal amplitude at Mauna Loa Observatory, global CO2 growth rate, and fluxes from model simulations and inversions are processed" page 5 line 2: replace "as seasonal amplitude" by "to define the seasonal amplitude" page 5 line 28: replace "was defined in Eq. (1):" by "is defined as:" page 11 line 33: replace "models’ mechanical difference" by "the differ-

ent parametrisations of important processes in models" page 12 line 21: replace "high latitude "greening" over high latitudes" by "high latitude "greening"" page 12 line 24: replace "to differ for different models" by "to differ between models" page 27: replace "in the S2 experiment (changing CO2 and climate and land use/cover) substracted by trends in S1 (changing CO2 only)" by "in the S3 experiment (changing CO2, climate, and land use/cover) substracted by trends in S2 (changing CO2 and climate)" Please proof articles. They are missing quite often.

Many thanks for your careful checking, we have corrected all the above mentioned typos (in a few cases not exactly as suggested, in order to retain original meaning). We also did another round of proofing and made numerous minor changes in the text.

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